THE PALESTINE EXPLORATION FUND

NOTES AND NEWS.

THE present number of the *Quarterly Statement* is restricted to a few pages of letters, and is issued a fortnight before the usual time. This is (1) because it is necessary to announce the public meeting of the 22nd, and (2) because the number will be supplemented by the addition of a paper long promised on the whole of the work of the Society from its commencement in 1865 to the present time, and the Biblical gains resulting from that work. A copy of this memoir, which is uniform in size with the books of Conder and Schumacher, and can be bound like them, will be presented to every subscriber who writes for it—free by post. It will contain (1) a *résumé* of the Society's history at home during the last twenty-one years; (2) an Account of the Excavations; (3) the Exploration of the Desert of the Exodus; (4) the Great Survey; (5) the Archæological Mission of M. Clermont-Ganneau; (6) the Interrupted Survey of Eastern Palestine; (7) the Geological Survey; (8) various Small Expeditions; (9) an Obituary Notice of former Members; and lastly, a clear statement of the Future Work open to the Society. The whole will be illustrated by woodcuts chiefly taken from the "Survey of Western Palestine." The writer of this history has had in view throughout the Biblical aspect of the work, and the gains to the student of the Bible.

On June 22nd, 1865, the first public meeting was held at which the Society was formally established. The Chairman on that occasion was the Archbishop of York. On the 22nd of June this year the Society will therefore attain to its twenty-first year. *Another meeting will be held under the same Presidency on the same day.* The meeting will be at the Theatre of the Royal Institution at 3 p.m. Admission will be by ticket only, which can be had of the Secretary; but early application is earnestly requested, as the Theatre will only hold eight hundred people.

The meeting will be addressed by the Archbishop, as President; by Mr. James Glaisher, as Chairman of the Executive Committee; by Sir Charles Wilson, Sir Charles Warren, and Captain Conder, as the Society's Exploring Officers; by Sir George Grove, Canon Tristram, Mr. Walter Morrison, Mr. John MacGregor, as original members; and by Professor Hayter Lewis, who will

describe the new discovery in Jerusalem. H.R.H. Prince Albert Victor has signified his intention of being present.

It is proposed to make this the occasion for raising such a sum of money as is wanted for the accomplishment of three objects, all of which urgently press, and should be carried through without delay.

These are, first, the Recovery of the Second Wall of Jerusalem. Professor Hayter Lewis, who has just returned from the Holy City, brings with him the news that the wall, whose discovery was reported in the January number, is a very ancient wall, 10 feet in thickness; that it is built of stones dressed in the way which is regarded as Jewish or Phœnician, namely, with the marginal draft. Such are the stones found by Warren on the lower courses of the Temple Wall; and such are those on the more ancient parts of "David's Tower." Further, there exists, outside the wall, a rock scarp which has been followed to the depth of 15 feet. The wall was laid open for 120 feet.

If, as now seems more than probable, this is none other than the Second Wall, its course, when ascertained, will either at once and irrevocably abolish the present so-called Holy Sites, with the Church of the Holy Sepulchre and the Tomb, or it will lend a new and most formidable argument to those who stand by the traditional sites. It may be stated that Herr Schick's opinion of its course places the Holy Sepulchre *without* the wall. There are many other considerations of great, though none of equal importance, connected with the Second Wall, which began at the Gate Gennath, "encircled the northern quarter of the city, and reached as far as Antonia," and probably contained the "Valley Gate," the "Corner Gate," the "turning of the wall," fortified with towers by Uzziah (2 Chron. xxvi, 9), and the "broad wall" (Neh. iii, 8; xii, 35). The course is now covered with houses, and tentative excavations will have to be made step by step.

The second object is the carrying out of the Inquiry into the Manners and Customs of the People now residing in and about the Bible Land.

The Questions have been drawn up for the Special Committee of Inquiry by Captain Conder. They are separated into classes, not only of subjects, but also of peoples, because there are many questions which may be put concerning the Bedawîn, for instance, which would be absurd to put concerning Greeks or Druses. Among the races and religions of Syria which will be subjected to this inquiry are the Greek Christians, Latin Christians, ordinary Moslems, Metawileh, Druses, Maronites, Armenians, Bedawîn, Fellahîn, Ansariyeh, &c. The Questions are framed in accordance with the scientific classification prepared ten years ago by Mr. Francis Galton and others for the British Association. A separate set of questions has been drawn up by the Folk-Lore Society for the Committee. As a good many questions are suggested by subscribers, Captain Conder, who has had all these placed in his hands, and has put together the Questions for the Committee in their present shape, desires it to be known that in some form or other all these suggestions have been acted upon, though in most cases the form has been quite changed and the inquiry made to lurk in another so as to avoid any appearance of a leading question.

Copies of these Questions may be seen at the office.

The remaining object is the publication of the materials now in the hands of the Committee. They would like to publish them for all members, but the cost is too great unless very substantial assistance is given them. The materials and plans consist of—

(1) Captain Conder's Survey of Eastern Palestine, with all his drawings and plans.
(2) Le Comte's Drawings, made for M. C. Clermont-Ganneau's Archæological Mission.
(3) Hart's Natural History Memoir.
(4) Schumacher's Second Survey (not yet received).

Professor Hayter Lewis has completed his second visit to Jerusalem. He employed himself in investigating especially the architecture of the Dome of the Rock. A few notes from him as to other points will be found further on. It is hoped that he will, before long, give the results of his study to the world. Mr. Laurence Oliphant will return to Syria in the autumn.

Captain Conder's new work will be out in October, published by Bentley & Son. Its title will be "Syrian Stone Lore." It will contain an account of Palestine and its people taken from the monuments, and will show us all that can be learned from them alone without the aid of the Bible. This is an entirely novel subject, and one which no one probably, except Captain Conder himself, is qualified to treat. As in previous cases, a considerable allowance in the price will be made for subscribers.

Captain Conder's article in the *Expositor* of May on Professor Socin's criticism of his work entirely bears out Mr. Walter Besant's notes on the subject in the *Quarterly Statement*. A few extracts from it are here reproduced, principally in order to show that the methods pursued by the officers of the Expedition were *exactly those which the German critic complained had not been adopted.*

Mr. Guy le Strange is compiling for the Committee a Gazetteer of Palestine from the early Arabic geographers. He expects to have this ready by the end of the year.

The Palestine Pilgrims' Text Society has issued its "Procopius," long promised. It is not published for the general public, but sent out to subscribers only. The Council's Report for the year 1885 is also ready, and can be had on application. Mr. Le Strange's translation of Mukadessy is in the press, and will now be ready in a few days. Mr. Walter Besant's translation of the Norman French Description of the City is also in the press, and will be ready very shortly for Captain Conder's notes.

As regards the copies which remain of the "Survey of Western Palestine," the friends of the Society are urged to get them placed in public libraries. The

work will not be reprinted, and will never be sold by the Committee at a lower price; and as it becomes known for the *only* scientific account of Western Palestine, it will certainly acquire a yearly increasing value. Five hundred were printed, and the type is now distributed.

The income of the Society, from March 17th, 1886, to May 31st, 1886, inclusive, was—from subscriptions and donations, £261 10*s.* 6*d.*; from all sources, £392 3*s.* 2*d.* The expenditure during the same period was £319 18*s.* 2*d.* On June 1st the balance in the Banks was £239 13*s.* 11*d.*

It is suggested to subscribers that the safest and most convenient manner of paying subscriptions is through a Bank. Many subscribers have adopted this method, which removes the danger of loss or miscarriage, and renders unnecessary the acknowledgment by official receipt and letter.

Subscribers who do not receive the *Quarterly Statement* regularly, are asked to send a note to the Secretary. Great care is taken to forward each number to all who are entitled to receive it, but changes of address and other causes give rise occasionally to omissions.

While desiring to give every publicity to proposed identifications and other theories advanced by officers of the Fund and contributors to the pages of the *Quarterly Statement*, the Committee wish it to be distinctly understood that by publishing them in the *Quarterly Statement* they neither sanction nor adopt them.

The only authorised lecturers for the Society are—

(1) The Rev. Henry Geary, Vicar of St. Thomas's, Portman Square. His lectures are on the following subjects:—

The Survey of Western Palestine, as illustrating Bible History.
Palestine East of the Jordan.
The Jerusalem Excavations.
A Restoration of Ancient Jerusalem. Illustrated by original photographs shown as "dissolving views."

(2) The Rev. James King, Vicar of St. Mary's, Berwick. His subjects are as follows:—

The Survey of Western Palestine.
Jerusalem.
The Hittites.
The Moabite Stone and other monuments.

(3) The Rev. James Neil, formerly Incumbent of Christ Church, Jerusalem.

(4) The Rev. George St. Clair, Bristol Road, Birmingham, has resumed his Lectures for the Society.

NOTES FROM JERUSALEM.

We are quite out of the world here, even the letters being cholera-bound, and when this will reach you I have no very clear notion ; but I thought it better to take the chance of getting a report to you of my doings. Dr. Merrill and Mr. Schick have been very kind, and given up much time to me, and Mr. Moore has rendered great service by securing for me a visit to the Hauran area without being limited as to time or any particular place.

1st. I wanted to secure an accurate account of the newly discovered piece of wall near here. I will mark out the exact position on your map. Meanwhile, I send a memorandum which I drew out and read over to Dr. Merrill, who quite approves of it. I think it is clear.

2nd. I have seen Mr. Schick, and marked out for him the precise places in which Sir C. Wilson and Mr. Birch wish to have excavations made.

Mr. Schick thinks that he may be able to get the fellahîn to do this, and will try. But this is Easter, when no one is working, so he will have to wait a little. Mr. Schick has also undertaken to trace further, if he can, the course of the newly discovered branch of the aqueduct at the suggestion of Sir Charles Wilson.

3rd. Russian excavations east of Holy Sepulchre. Of these I have made sketches with dimensions. I cannot explain them without.

4th. I have made a sketch plan of the very curious underground church to which Sir C. Wilson alluded.

5th. I have been able to examine carefully the Sakhrah and Aksa twice. I had made abstracts of all the chief points of dissension, and ranged them in columns so that I had everything clear, and Mr. Schick went with me and gave me some valuable information as to newly found details.

6th. The remains of St. Stephen's Church are well preserved, but the figures of saints have almost faded away.

Near the church some extensive excavations have laid open some fine Mosaic pavements, showing that a large Roman mansion had been there. I will get drawings if I can, but I doubt having the time.

The environs are being rapidly built upon, and many beautiful points of interest are being spoilt by ugly houses ; but the building has had one curious result, viz., to clear away the great heaps of rubbish near the Damascus Gate. These are rapidly being removed and laying the walls bare.

In respect of the masonry of this gate I noticed a curious fact which has no doubt been previously remarked, viz., that several courses of the flank wall east of the gate which appear at first sight to be formed of the same small stones as are used in the Arabic portion adjoining, are really

formed of such massive stones as were used in the old wall work, but tooled down and chiselled across with fictitious joints so as to assimilate with the Arabic.

The outside of the newly discovered wall (opposite to Frutiger's bank) was to the west, viz., next to the "open field" as shown on the ordnance map, and is now being built upon to form one wall of a new hotel.

If continued, this outside line would strike the Tower of David at about three-sevenths of its width from the west front, and, so far as can be seen, at right angles.

The wall was composed of large stones not less than 6 to 8 feet long, and about the same height as the lower courses of David's Tower, and drafted like them; and the masonry, altogether, resembled that of the Tower in every particular. The wall was partly uncovered for a length of 120 feet, and was found to be one, two, or three courses in height, the lowest being directly on the rock.

Dr. Merrill saw the wall for a thickness of only 3 or 4 feet, but Mr. Schick happened to pass when it was uncovered for its whole width. He did not measure it at the time, and it was covered up when he returned, but it was certainly 8 to 10 feet thick.

The interior face of the stones was worked smooth.

Beyond the wall, externally, was a scarp in the face of the rock which was exposed for a depth of about 15 feet.

The new east wall of the hotel was required, owing to the street being widened, to stand partly outside of the old wall.

T. Hayter Lewis.

Mediterranean Hotel, Jerusalem,
26th April, 1886.

CAPTAIN CONDER'S REPLY TO PROFESSOR SOCIN.

(*From the "Expositor" of May*, 1886.)

THERE are three questions in Professor Socin's paper which have perhaps hardly been sufficiently distinguished. First, that of the permanent Map and Memoirs by which the Society must be judged. Second, their more ephemeral or speculative publications, the *Quarterly Statement*, and the Old Testament and New Testament Maps, with their popular books. Third, works not published by the Society at all, such as my Handbook and Primer, and Mr. Henderson's Handbook, for which the Society are in nowise responsible.

It is only fair to the Committee of the Society to remember that library scholars and the conductors of exploration parties are not made always of the same stuff. The Committee choose the commander; they ask him for a professional report, whereby he must be judged; and if he choose to add the results of his own literary studies, and if they publish these always with the caution that for such suggestions the author is responsible, it is, I think, clear that they have fulfilled at once their duty to the public and to the explorer. This is what the Society have always done. It is a question then: 1st, as to the professional report; 2nd, as to the explorer's opinions; but in both cases a question between the explorer and the critic, not between the Committee and the critic. I think that within the limits at his disposal, Professor Socin might have said more than he has about the professional reports—as to the physical description of the country, the minute accounts with plans and photographic drawings of the ruins, the legends and notes as to population, the inscriptions, and other details tending to establish date or historical sequence, the accounts of masonry dressing and other distinguishing peculiarities. Professor Socin has, however, preferred to confine his notice to picking holes in the results which have been published outside this professional report on Palestine, which forms the main material of the Memoirs, and the most solid basis of the reputation which the Palestine Exploration Fund enjoys, at all events in England.

I am not aware that the permanent publications of the Palestine Exploration Fund can justly be said to have, what he calls, an "apologetic tendency." Survey and the description of ruins have no tendency at all; they represent the collection of facts on which the reader may put any construction he pleases. The strength of the Society lies in the fact that officially it recognises no *views*, but only deals with ascertained facts. It is clear, from Professor Socin's misconstruction of my views on Biblical criticism, that there can have been nothing in the Survey Memoirs to allow of his knowing what those views are.

As regards the identification of the Akkadians with Mongols, I am not sure what Professor Socin's objection can be. Perhaps I should have written Finns or Uralo-Altaic races, but this is a very slight alteration.

I can hardly believe that Professor Socin is ignorant of the results of philology in this case. The labours of Lenormant have proved beyond doubt that the old non-Semitic speech of Mesopotamia of the Akkadians, Sumerians, early Elamites and Cosseans, was closely akin to the Finnic language, and (according to the ordinary use of the word) was therefore Turanian.[1] Again, as regards the Amorites, Professor Socin says, "It is the name in a particular document for the Canaanites in general." I presume he is referring to one of the hypothetical documents into which some German scholars divide the Pentateuch; but considering how various are the views as to these components, no ordinary student is as yet bound to accept any one among them in particular as belonging to the category of ascertained fact. Professor Socin is presumably aware that the Amaur are mentioned in Egyptian inscriptions, and I may remark in the known instances that they always appear as inhabiting the "high lands."

Professor Socin again seems to fail in making a point as regards the worship of the Makams, "a worship," he says, "as different from the old idolatry as is the Catholic image worship." Has he, I would ask, reflected on the mass of evidence which shows that Catholic image worship is directly founded on paganism, and that throughout Europe pagan deities of the Kelts or Gaul or Germans are still adored as Christian saints? The parallel is at least an unfortunate one for the critic.

As to the acceptability of Talmudic tradition in topography, there may be differences of opinion. My own belief is, that the earlier works of the second and third centuries, included under this general title with others of later date, are of very high value, as representing indigenous tradition. Like all other evidence, it cannot of course be accepted unquestioned. The question of identification is again one of opinion, but the rules of the interchange of certain letters which I have always attempted to follow are recognised by every student. Why Professor Socin should prefer Talluza (طلّوزه) to Teiasir (تياصير) as representing Tirzah (תרצה) I cannot see; the former word has not a single letter in common with the Hebrew.[2]

[1] If this "makes a very painful impression on a serious German student," I can only suppose that the student in question knows very little of Assyriology. As to the Phœnicians coming from Mesopotamia, the evidence is not only that of Strabo or Herodotus, but includes philological considerations which seem to me of great weight, such as the name Akharu, the worship of Nergal and Tammus, and other indications of a like kind. I am aware that this migration is doubted by some, but it is accepted by good authorities.

[2] As regards the cases (four out of more than 150) to which Professor Socin takes exception—

Hosah = Ezzîyah is suggested for topographical reasons.

Hannathon = Kefr 'Anân is also chiefly on account of geographical position. The Talmudic Caphar Hananiah seems, however, to give an intermediate stage.

Neiel has the article in the Hebrew, which Professor Socin seems to neglect.

As regards the Tomb of Rachel, I can only say that I have no confidence in Mr. Schick's supposed discovery. I investigated the matter carefully on the spot and found no basis for his assertion, nor has any one since confirmed the supposed existence of the name at Mr. Schick's site. Nor do the passages mentioned by Professor Socin (1 Sam. x, 2; Jer. xxxi, 15) prove that Rachel's Tomb was ever north of Jerusalem, while Gen. xxxv, 19, not mentioned by Professor Socin, distinctly states that this tomb was near Bethlehem (see 16).

Professor Socin does not believe that the Cities of the Plain were north of the Dead Sea. Josephus said they were under it, and the Biblical account may mean the same, but I can hardly think that any one who has visited the southern shores of the Dead Sea could believe it had ever been a district capable of supporting a settled population, whereas the plains of Jericho still are so capable. This, however, is not a matter in any way affecting the credit of the survey of Palestine.[1]

As regards my identifications of Neby Namân with Micah, and of Neby Mashûk with Melkarth, Professor Socin has omitted all reference to the historical evidence on which alone they rest. Perhaps he has not been able to find it in the Memoir, but I assure him that it is there awaiting his perusal. I am perhaps to blame for not giving cross references, but must beg for indulgence, as I was again exploring in Palestine while the memoirs of my first survey were being published in England.

I now come to the question of the Arabic name lists, where I am more in accord with the critic. I cannot, however, think that Professor Socin can have read my account in the first volume of the Memoirs of the Method of Execution of the Survey. It is hardly possible that he can mean flatly to contradict—without any personal knowledge of the survey operations—my direct statement to the effect that the names were *never* repeated by the surveyors to the scribe. Each surveyor had with him invariably a local guide. Every name was taken down from the mouth of that guide in my presence, and in that of the surveyor, by the scribe. The error, if any, must have been that of the native guide. I do not, however, note any instances of such error mentioned by Professor Socin, and I have no doubt that my assurance will induce him not again to repeat his hasty assertion, which is contrary to fact.[2]

Tell en Nahl is quite out of the question, but I am not responsible for this rather wild shot of Mr. T. Saunders.

Chephar Haamonai is also supported by topographical requirements as to situation.

[1] The suggestion that Kasim was Cadmus was made by Professor Palmer. It certainly seems unfounded. As to Jisr Mujâmia' there is a legend attached to the bridge, of a great gathering which once occurred there.

[2] There is one instance in the north where the name Tireh is spelt تيرة, yet translated "fortress," by Professor Palmer. I was, I believe, the first to show how this Aramaic word طيرة (טירה) survives in Palestine, though its

The grammatical points raised by Professor Socin do not show, as he supposes, our ignorance of Arab grammar. They evince clearly to any one who has for six years[1] been living among the Fellahîn, writing down their words, inquiring into the peculiarities of their dialect, and with the aid of experienced natives and residents examining the question of nomenclature, that Professor Socin has himself very little knowledge of these dialectic peculiarities. Had he possessed such knowledge he would not have prepared a vocabulary of "townsman's Arabic" only, for his travellers, and he might even be puzzled to understand a fellah of the outlying districts when he spoke. Thus, for instance, Burâk is no doubt not the proper plural of Birkeh, but it is certainly a form used by the peasantry, as is Buwâb instead of Abwâb for gates. The correct form was constantly suggested to me by our scribe, but I always insisted on the fellah form being that written down. The various sounds of the feminine ending faithfully reproduce the fellah intonation. Surely Professor Socin does not suppose that Professor Palmer was capable of ignorance on such an elementary point as that of the *status constructus*, and I may tell the critic, that the list of abbreviations and explanations for those who could not read Arabic, was prepared, not by me, but by Professor Palmer. The question of transliteration is one of very secondary importance. Robinson's earlier method was adopted by the Committee because it was familiar in England. It is not in itself a good system; but no student would rely on the English lettering when he could find the original Arabic in the name lists.

The critic again objects to the translation of Shem (שם) as meaning "brown." It is not, however, my ignorance which is thus shown. The careful note on this question by Professor Sayce in the "Proceedings of the Biblical Archæological Society" seems to me to leave no doubt on this subject. This is one of several instances in which I think Professor Socin hastily condemns statements as to the foundation for which he knows nothing.

As regards other writers, it is not my business either to defend or to condemn. The contributors to the *Quarterly Statements* of the Society are of various calibre. It is not I think undesirable that, in an ephemeral production of this kind, all who wish should find room to write, but the value of their contributions is matter of opinion. Personally, I should prefer not to see its columns filled with endless discussions on unimportant points which can probably never be settled. I should prefer not again to read therein bad jokes, or personal details of ordinary travellers' mishaps; but these are rare and unimportant details, and no doubt much very

meaning is lost to the natives. They translate it "bird" (طير), and in the same way Râmeh ("the hill") they translate "the tank."

[1] I spent the years 1872, 1873, 1874, 1875, 1881, 1882, among Fellahîn and Arabs. I do not know how many years Professor Socin includes under the term "a considerable time," nor do I know if he actually lived among the peasantry and conversed with them daily in their own language.

valuable information has been obtained from outsiders through the columns of the *Quarterly Statement.*

I would make an exception in the case of the Rev. A. Henderson to the criticisms of Professor Socin. This writer has always been remarkable for moderation, modesty, and freedom from prejudices. We may not always agree with his views, and I hear, in corresponding with him, that there are a few slips and printer's errors in his Handbook, which he expects to amend in a future edition. I think, however, that this work—which has, by the bye, nothing to do with the Palestine Exploration Fund—is generally so sound and useful that it cannot fail to be acceptable in the class for which it is intended.

We have come thus to the end of the Professor's criticisms. We must thank him for his expressions of approval, and also for a good many really valuable suggestions and objections, but at the same time we may fairly expect him to withdraw many others which are hasty and ill-formed. We may also be allowed to suggest to his consideration, that no work—not even his own—undertaken by mortal man is perfect, and that it is necessary to look at the general character in pronouncing a verdict. The task of exploring 6,000 square miles, and then preparing and publishing the results, is not a small task. It has fallen mainly on the shoulders of Mr. Walter Besant and of myself, though there have been many distinguished contributors. I have no doubt Mr. Besant feels as I do, that we have learned as we went on. The task of final assimilation of the huge mass of material is not yet complete. It will probably not be complete for many years. I hope soon to offer a contribution to such assimilation in a work on which I am still engaged; but I fully expect to see, even in Professor Socin's future editions of his Handbook, the influence of the work that has been already done. Professor Socin's time is, no doubt, mainly occupied by original research rather than by criticism, and we may hope to obtain some results which may be more valuable even than his critical comments on the Palestine Exploration Fund, from the labours of the German Palestine Society. As yet we have had nothing very striking from them either in the way of exploration or of literature. The papers by Herr Schick and his plans are welcomed as the work of an old and zealous workman, but they are open to criticism far more severe than that levelled against the English Society. After all, we have given the public a solid mass of information, vouched for by professional men, and accepted by students of a very high class in England.

The critical school is fast being superseded by the historical in England. The study of monuments and inscriptions, coins, statues, and buildings, gives us more certain results regarding the vexed questions of Oriental antiquity than any amount of exegetical criticism can be expected to give. If Professor Socin doubts the existence of non-Semitic races in Syria, his doubt is not shared by those who have studied the records of Egyptian and Akkadian monuments, and I for one believe that more is to be learned from such comparative study than from any amount of theorising on "documents," "editors," "first and second Elohists," and

the rest; at the same time it does not follow that because our line of research leads away from these bitter controversies to the safer path of contemporary monumental evidence, we are therefore ignorant of what has been written in these matters. I have studied the works of Kuenen, Ewald, Colenso, Robertson Smith, and other critics, and have become generally acquainted with the views of Hitzig, Wellhausen, and other German critical writers, and I have read Renan's great work, as well as numerous books of Lenormant; but there are many other branches of study which must yield their contributions to the study of Syrian antiquity, and to which Professor Socin does not refer. Such are the publications of the Biblical Archæological Society, the "Records of the Past," the Sacred Books of the East, the works of Smith, Layard, Rawlinson, Boscawen, Taylor, Sayce, Chabas, Brugsch, Birch, Mariette, De Rougè, and many more. There is so much to do in collating all that these great scholars have written respecting Syria, that the study might well fill a lifetime without leaving time for exegetical works. I think Professor Socin will agree, that time is better spent in trying to learn than in trying to pick holes in other men's work. As regards the word already spoken—that is past. If there has been error or shortcoming, all that can be done is to amend in the future, and to strive through the aid of one's critics to avoid the perpetuation of error. In the end, the true lives, the false dies away. All we have a right to require of every writer is, that he should be honest, well-informed, and open to conviction, conscientious in doing his best, and conscious of his own fallibility.

STONE DOORS.

I.

We have been long familiar with the Stone Doors of Bashan, but Mr. Oliphant's discovery of a couple of these old portals, built into the house of a Jew at Tiberias, gives a new interest to the subject. In Palestine stone doors belong to the past, but I write to state that while lately accompanying the Afghan Boundary Commission through Persia I saw doors of that kind still in use at the present day. The necessity for them in that country may, perhaps, throw light on the conditions which required such protection in Palestine in times gone by. For centuries back, and up till only about two or three years ago, Persia has been liable on its north-eastern frontier to incessant inroads from Turkoman raiders; these raiders came at times in small bodies, at times in large numbers; their plan of action was necessarily hurried, they swooped down on villages and carried off whatever they could pounce upon. Men, women, and children, as well as houses, cattle, and sheep, were all prey to the Turkomans. The human spoil in such cases were carried off and sold as slaves in the

bazaars of Khiva and Bokhara, where almost all chance of escape was cut off from the great desert of Central Asia being thus placed between the victims and their own country. The Persians had to protect themselves from such inroads as best they could, and with this object every village was walled, towers of refuge were erected in the fields: these had a narrow passage to creep up through, so that if a raider tried to follow, the refugee could batter his head as he emerged upwards. On similar towers watchmen were placed on the look-out, and when an "Alaman," as such forays were called, was seen approaching, guns were fired and every one rushed, either to seek protection within the walls of the village, or, if that was too distant, to find safety in the nearest tower of refuge. These raids were sudden, and rapidity was the essential part of the tactics. If the rush on a village failed, there was no delay to make an attempt by other means; the party darted off at once to try their luck at the next village before news of the foray could reach it. Such being the case elaborate defences were unnecessary; shelter for the moment was all that was required. It would not have taken long to burn a wooden door, and thus gain an entrance where there were few defenders. The villages could defend their gates from being burned, for they had loop-holes so placed as to protect them; but in some of the larger towns there were houses with gardens outside the walls. The walls in such cases were high, and the only entrance was by means of a small stone door. There was a defensive strength even in smallness, and they were often of most diminutive size, some being less than 3 feet in height. I only saw one village which had a stone door. This place is called Lasgird, about 100 miles due east of Tehran. It is a very curious spot; the outer wall is a circle, but it is a thick mass, being wide enough to contain the houses of the villagers in it; these are in the upper part, about 30 feet from the ground, where the people are out of danger from attack. There are rude balconies all round, by means of which the people can communicate from house to house; the central space within the circle has houses for the storage of grain and for the horses, cattle, &c., and there is only one small gate to this place, with a massive stone door, the dimensions of which are 45 by 37 inches and 7½ inches in thickness. The village is supposed to be very old, and the tradition is that the circle, or "gird," which forms its plan, was originally traced on the ground by *Las*, or *Last*, the son of Noah. This character will no doubt be new to most readers of the *Quarterly Statement*,—he was so to myself; in all my travels in the East I never heard of such a personage. Most probably there may be other legends regarding him, and I for one would feel obliged if any person can throw light on this fourth son of Noah.

This stone door of Lasgird is a very rough piece of workmanship; I have put the thickness at 7½ inches: this was about its greatest depth, but it was so rudely made that this varied considerably. It wrought on pivots, the same as the doors of Bashan; the smaller doors I have described did the same. It will be noticed that the stone doors of Bashan, as well as the pair of doors found at Tiberias, have been copied from

models which had been beautifully fashioned in wood: the panels on them are clear evidence of this; they had also been ornamented with metal bosses. The stone doors of Persia speak for themselves, and tell of danger from sudden attacks. This suggests that the transformation on the east of the Jordan from wood to stone implies a change which had taken place at some former date in the condition of the country, that a peaceful state had been succeeded by an unsettled period, during which the use of stone for doors became, as we find in Persia, a necessary precaution. The substantial form of the original wooden, as well as the elaborate reduplication of them in the lithic material, is not consistent with the idea of a hasty or momentary adoption, but points, in both cases, to a considerable duration of time when each of them had been in use. We may thus derive a hint from these peculiar portals which may be of some historical significance.

WILLIAM SIMPSON.

II.

THE STONE DOORS OF TIBERIAS.

THE stone door at Tiberias figured and described on page 79 of the last *Quarterly Statement* is the same that I saw in 1880, and which I described in my "Sacred Palmlands" in the following words:—"*Tiberias, March*, 31 I long to know the date of a pair of massive old stone doors which we saw inserted into the gateway of a modern Jewish house as we passed through the town this morning. No sooner had we stopped to notice them than the Jewish proprietor, accompanied by a number of his relatives and friends (belonging to the Askenazim and Polish Jews), came out and gathered in a group around us. They told us that the doors had lately been discovered buried under the present edifice. They are carved in imitation of wood, the design on them being bosses arranged quincunx fashion, alternating with panels. In the upper inside panel is a projection hollowed out apparently for the reception of the bolt which kept their massive folding doors securely fastened. I observed a block of stone also with bosses carved upon it, built into a house further on, and other remains of this sort are to be found here. I fancy by the style of workmanship they belong to the same period as the giant cities of Bashan." The sketch of the stone door made on the spot by my mother (who was travelling with me) is identical with the engraving on page 79 illustrating Mr. Oliphant's paper.

A. G. WELD.

DID THE WATERS OF THE JORDAN ORIGINALLY FLOW INTO THE GULF OF AKABAH?

A Rejoinder to the Writer in the "Saturday Review."
By Professor Hull, F.R.S.

The mode of formation of the Jordan-Arabah Valley has been a subject of investigation, or of speculation, for a long period of time. The idea, first, I believe, suggested by Burkhardt, that the river Jordan originally flowed down the whole course of the depression from the Lebanon to the Gulf of Akabah, has been deliberately rejected by Lartet, and more recently by myself, after an examination of the line of depression from the Gulf of Akabah to the plains of Jericho and the Jordan Valley. This hypothesis has, however, been revived by a writer in the *Saturday Review*,[1] who it may be presumed has not had the same opportunities of personal examination of the ground, and who takes the author somewhat to task for presumably not giving this view sufficient examination in a recent memoir on the geology of this region.[2] The Reviewer objects that I leave unexplained "the peculiar and distinctive contours of the Jordan-Arabah Valley, especially the gradual descent on both sides to the greatest depth in the bed of the Dead Sea, as well as the singular flattened watershed in the Wâdy Arabah." The writer proceeds: "The physical features of the region appear to accord far better with the view that the valley as a whole marks the course (originated no doubt by a line of flexure and faults) of a river which once poured its waters into the sea on the site of the present Gulf of Akabah, and that this valley was afterwards affected by a new series of flexures running east and west, the result being to raise the parts to the north of the Gulf, and to lower the whole of the upper region, the maximum depression being now indicated by the Dead Sea. This hypothesis, the more natural, as it appears to us, does not appear to have been duly considered by Professor Hull."

It is perfectly true I have not devoted much space to the attempt to confute an hypothesis which I believed to have been universally abandoned, and which, as I hope to be able to show, the more it is examined in detail the less does it appear to be capable of support. The enormous difference of level between the floor of the Dead Sea and the summit of the Arabah watershed, amounting to over 3,270 feet, seems to me an insuperable objection to the view that the Jordan-Arabah Valley could ever have been a continuous river-valley.

There is undoubtedly, at first sight, a great temptation to infer the existence of a continuous river-valley, when one looks at a physical map

[1] *Saturday Review*, 17th April, 1886, p. 552.

[2] "The Survey of Western Palestine: Memoir on the Physical Geology and Geography of Arabia Petræa, Palestine, and Adjoining Districts." Published by the Palestine Exploration Fund (1886).

of the entire region. At the northern extremity, where are situated the sources of the Jordan, we behold the highlands of Syria; at the other an arm of the sea, and each connected by a nearly continuous valley. I say *nearly* continuous, because at the saddle (or watershed) near the centre of the Arabah, the valley is mainly represented by the highlands of Edom on the east side; while, on the west, it is divided into two by a very low ridge of limestone called Er Rishy. Indeed, at this critical point it is somewhat difficult to determine where the real valley lies, as the western arm opens out upon the great plain of Badiet-et-Tîh, and the eastern forms a terrace covered by gravel stretching across from the limestone ridge to the base of the Hills of Edom.[1] It seems to me that any one who studies the features of the watershed, as they are represented on the topographical map of Major Kitchener and his assistant, Mr. J. Armstrong, will come to the conclusion that they are very unlike those to be expected in the case of a large river-valley near the termination of its course. In fact, the distinctive contours, which are so remarkable in the Jordan depression to the north, and also in the southern portion of the Arabah Valley, here almost disappear.

Now, I come to inquire, what are these "peculiar and distinctive contours" which are so much relied upon by the Reviewer for indications of a continuous river-valley? I presume they are only applicable to the western margin, because those of the eastern are clearly and undoubtedly due to the elevation of the strata along the line of the great Jordan-Arabah fault, which was traced by us from the shores of the Gulf of Akabah to those of the Dead Sea, a distance of about 120 miles. To the east of this fault the different rocks crop out, and the sedimentary strata, having a general dip towards the east, break off in cliffs and escarpments such as are generally observable in similar cases.

I must therefore assume that it is in the scarped cliffs of limestone along the western side of the Jordan-Arabah Valley that we have the supposed indications of a river-valley margin. These cliffs are undoubtedly often very imposing, lofty, and precipitous. Taking the portion of the valley called *The Ghôr*, in which lies the Dead Sea and the Lower Jordan, if I am asked how do I account for these cliffs on the one hand, and for the deep hollow of the lake basin on the other, I have to reply, that the cliffs were formed in the first instance by wave action along strata fissured and fractured by faults, and subsequently by meteoric abrasion. It must be recollected that, owing to the manner in which the stratification is concealed along the sides and bottom of the valley by lacustrine deposits, it is seldom possible to make observations on the Cretaceous limestones; but, judging from the manner in which the beds are in some places flexured and highly tilted towards the western margin of the Ghôr, it is a fair inference that there are, besides the main fault which generally runs at the eastern margin, numerous other faults or fissures, by which the beds have been dislocated and displaced. When referring to the line of the

[1] Geological Memoir, p. 20.

Jordan-Arabah fault as an axis of disturbance, I have observed, "it is sufficiently clear that this line was an axis of disturbance for the whole region now under consideration. Along this line the strata are either displaced by secondary faults, or contorted and tilted at various angles; while, as we recede from the line of the displacement, the beds generally begin to assume a position approaching the horizontal."[1]

Again, as regards the deep hollow of the Dead Sea basin, I have stated that as the land area was gradually rising from beneath the Miocene ocean, and as the table-lands of Judæa and Arabia were being more and more elevated, the crust fell in along the western side of the Jordan-Arabah fault, and that the bed of the trough, owing to continued subsidence, became more deep over the area now occupied by the waters of the Salt Sea itself, and that into this gulf all the waters flowing from the bordering lands naturally emptied themselves. In a word, my explanation of the existence of the deep hollow of the Dead Sea is:—unequal subsidence along the line of the main fault, and the presence of parallel and transverse fractures along the western side of the Ghôr; and if we recollect that this valley was filled by the waters of an inland lake, 200 miles in length and 8 or 10 miles in breadth, we shall have no difficulty in accounting for the presence of an aqueous agent, powerful enough to undermine, and in undermining to shape the lofty cliffs which abound the valley in some places.

With reference to the fine cliffs, often in the form of a double scarp, which bound the western side of the Arabah Valley for about 30 miles to the north of the Gulf of Akabah, I may observe, that they are essentially similar in character to those along which the Cretaceo-Nummulitic limestone of the Plateau of the Tîh terminates towards the south and west, where the idea of river action is out of the question. During the period when the land was rising out of the sea, and again during the period of subsequent depression, when the sea rose from 220 to 300 feet, these cliffs must have assumed *in the main* their present outline. This has subsequently been modified by rain, frost, and water action; but I can see no sufficient reason for inferring the action of a great river in the tract of the Arabah south of the Dead Sea, nor do we find the remains of river terraces in the Arabah Valley, such as we might have expected if that valley had been the bed of a large river, which had afterwards dried up owing to subsequent physical changes.

Lastly, turning to the hypothesis of the *Saturday Review*, that after the formation of the main line of depression along the great north and south fault there occurred transverse movements, producing east and west flexures, I am quite unable to accept such a proposition. The general displacements of the strata were produced during the Miocene epoch, and except the subsequent gentle lowering of the whole region in the succeeding Pliocene, to the extent of 200–300 feet, which was unaccompanied by flexing of the strata, I hold that all the great disturbances are referable to the one epoch, namely, the Miocene.

[1] Memoir, p. 107.

The lowering of the deep trough of the Ghôr to a depth of 3,000 feet along an east and west synclinal axis could not have taken place without affecting the levels of the table-land of Southern and Central Palestine to a very appreciable extent. But, as a matter of fact, the central table-land is as a rule quite as elevated, or somewhat more elevated, to the west of the Ghôr as it is either to the north or south of that position. Nor does the existence of the saddle or watershed of the Arabah, which is 700 feet above the sea, seem in the least to affect the general levels of the table-land of the Badiet-et-Tîh, for, as already stated, this saddle seems to merge into the general surface of the country along the west.

One other point remains. Had the streams on each side of the Arabah entered a river flowing southwards it may be assumed that their channels would have pointed, more or less, in the same direction, at least along that part lying north of the watershed. It is only necessary to refer to the map of the Arabah to see that such is not the case. The valleys of the Jeib and its tributaries, together with those on the Edomite side, will be seen in the great majority of cases to point more or less to the northwards, indicating an original northward flow.

Having thus given the hypothesis of the Reviewer as full a consideration as appears necessary, I trust I have made it sufficiently clear, from considerations based on the physical features and geological phenomena of the region in question, that this hypothesis cannot be sustained.

"THE VALLEY OF ZEPHATHAH AT MARESHAH."

Captain Conder in his April notes on the *Quarterly Statements* of last January referred to my article of the above title, and said, "The ingenious suggestion of Mr. Flecker does not seem to me necessary." The reason he assigned for this opinion was that he had some time ago proposed what he thought to be a satisfactory identification, viz., Wady Safieh, which I confess I have neither seen nor heard of before. As, however, readers of his remarks might think that I had invented a pretty theory, and rushed into print to overthrow better suggestions for the sake of my own, I begin with—

I. Some explanations of my suggestion. It pretends to no ingenuity, for I have invented nothing. I have only given the reading which I found in the LXX. I may have been the first discoverer of the cause of the variation, for Schleusner does not give it. But that discovery is so simple that it is merely for want of attention that others did not see it before me. The only thing that might be blamed or objected to in my suggestion is the proposing a Septuagint reading instead of the Masoretic Hebrew. To this I can say that I have not done it because I like to correct the Authorised Hebrew by the Greek Version, but because, in

spite of my disinclination to do so, and in spite of my firm conviction that the Hebrew Scriptures have been preserved with an exactness and freedom from error and corruption which are unparalleled in any work of literature of any antiquity, I could not help being struck with the smallness of the change and yet greatness of the improvements which that change introduces into our passage of 2 Chron. xiv, 10. The two readings as seen in the Hebrew letters צפתה and צפונה differ from one another by a space of the size of a point which divides one of the letters and makes it into two. Now, however low you may estimate the difficulties of the Masoretic reading, which gives the one undivided letter, does it not seem strange, if that little space in the Alexandrine reading is an error, that it alone and nothing else should be able to smooth away all these difficulties, and introduce improvements which satisfy completely the requirements both of the language of the text and of the locality of the history? If this error or slip of the pen could really be proved, then the least we can say is that it is a very interesting error, and therefore even then it would not be unworthy of notice.

It is remarkable that another most obscure passage of several verses in Zech. xi is also most beautifully cleared up by a similar but reverse mistake (if the mistake is indeed on the LXX side); viz., the annulment of a space between two words making them into one. As that passage now stands in the Hebrew, no commentator has yet been able to make good sense of it that is not forced. Take especially the word לכן of verse 7. Commentators have invented various theories about it, not one of which has been generally accepted as satisfactory. The Revised Version, following Pagninus' *profecto*, translates it "verily," which can only be justified by the supposition that לכן stands for אכן. But what says the LXX? It only abolishes the space between this and the following word, reading לכנעני (or strictly speaking לכנען) instead of לכן עניי, as also in Verse 11, כנעניי (or כנענים) instead of כן עניי.[1] Yet this slight external alteration has an immense effect internally. It removes the disappointing anti-climax, "verily the poor of the sheep," and such like renderings; it gives a new complexion to the whole passage; and it introduces good sense instead of confusion, a connected account instead of broken and abrupt sentences. When we further consider that the Greek Translators themselves did not see the improvement, nor understood the passage at all, which shows that that small but important alteration we are speaking of, was not an intentional correction, but a rendering according to their reading; can we reject this also as an error not worth proposing? Now the meaning of that passage thus corrected I take to be this. Just as Isaiah and other prophets were sometimes commanded by God to act

[1] It should be remembered that there was no final Nun in those days. See my "Scripture Onomatology," p. 25, and "Smith's Dictionary of the Bible," p. 1206.

their prophesies before they spoke and explained them (see Isa. xx; Jer. xiii; Ezek. iv, v, xii, &c.); so here.

Zech. xi, 4, 5, Zech. is first asked to act, viz., to shepherd butchers' sheep, or "sheep for slaughter whose buyers kill them and are not condemned, and whose sellers," as well as shepherds, do not pity them, the former rejoicing in the money they have obtained for them.

V. 6. This is explained at once to mean that God will not spare the inhabitants of the land, even as men do not spare such cattle.

V. 7, 8. Then the prophet obeyed the command, shepherding sheep for some sheep merchants (לכנעניי הצאן, not εις την χαναανιτιν—LXX). He also took two staves for the purpose, and wandered away (lit. cut himself off) with three other shepherds for one month, when he became sick of his companions, and they also detested him.

V. 9, 10. Then he said, "I will not be shepherd with you; let the dying die, and let the wandering wander away," &c. He also broke one of the staves as a sign that he had broken every covenant that he had made with any one.

V. 11, 12. Then those merchants (כנעניי) who kept him or hired him "knew that it was the word" or commandment of Jehovah. Therefore, when the prophet asked whether they would give him his hire; though he neither did nor could insist by right upon it, because his covenant with them was broken, yet they gave him 30 pieces of silver.

II. To return to our subject of 2 Chron. xiv, 10; as regards Captain Conder's new identification, if I am not mistaken, it seems but to describe by a different name the same valley to which the LXX also points. He calls it Wâdy Safieh which "passes quite close to Mareshah," and "up which runs one of the high roads to Hebron;" and the LXX calls it the valley north of Mareshah. If this name is correct, it stands in opposition to that of the southern valley which now bears the title Wâdy el Arab, but was most likely in olden times termed the valley south of Mareshah. But why does the Captain call the northern valley Wâdy Safieh? The map does not give it. It shows a broad strip of low country branching out into W. el Judeiyideh with a road running straight north, W. esh Sherkiyeh with the Jerusalem and Hebron road, and W. Fedeil which seems to be a shorter cut to Hebron. I can only think that he applies the name Wady Safieh to the beginning of W. esh Sherkiyeh up to or a little beyond where it passes Kh. Safieh. But do the natives also call it so? And if they do, may it not be a modern name, even as W. el Arab is in the south?

Further, the Hebrew too, points, in my opinion, clearly to the same spot, viz., the valley or valleys above described. For the valley had to be north on the way to Jerusalem and near to Mareshah (למרשה). Yet the large armies which were engaged in that historical battle may have filled miles of those valleys. If, then, identifiers do not find a name resembling Zephathah on that spot, it is out of the question to look for it on any other. Now, it so happens that there is Khurbet Safieh. But the

Bible speaks of a valley, therefore we want a Wâdy, not Khurbet. This, we have seen, Captain Conder professes to have found too. Still I hold that an identification in order in any degree to accomplish the aim of his identification, viz., to render that remarkable LXX reading unnecessary, ought to have some stronger certainty about it than the unmapped Wâdy Safieh has; and the affinity or kinship of its name ought to be more real and substantial than that which exists between צפת(ה) and צאפי(ה). For though in the history of names these and greater changes may in some unaccountable way happen to take place, yet it is not a usual thing at least that a ת should turn into a י.

But, finally, when we also consider that no identification can smooth the language of the text, then we see that the Hebrew cannot by such means be satisfactorily established, nor the Greek really proved to be in error. The question, therefore, resolves itself of necessity into one between the two readings. Perhaps another visit to the place itself will do much to clear up matters concerning the valley. But, with regard to the two readings, I do not wish to be positive in my opinion about them, but am content to say that there will probably be no assurance upon the question between them until the day when this time of uncertainty shall have passed away, and we shall know even as we are known.

E. Flecker.

Durham, May, 1886.

ZION, THE CITY OF DAVID, OR ACRA, SOUTH OF THE TEMPLE—*continued.*

I observe that Sir Charles Warren makes no attempt to defend his site for Zion. Perhaps he is too busy, or rather, too wary to reply to my arguments.

He claimed the *link* in support of his site, and in the last statement Captain Conder has appealed to the *chain* in opposition to mine. Our rival theories can be submitted to no better test. Accordingly, though it may seem presumption in me to contend against these professional surveyors, I venture to try their theories by the *link and chain*.

Sir Charles Warren (Temple or Tomb, 22) admits that Zion = the city of David = the Macedonian Akra = the Akra of Josephus, and observes, "Therefore we have a connecting link throughout." Precisely so. But I have proved (*supra* 26) that the Acra of Josephus was south and not west of the temple. Therefore the *link* gives its support to my theory, not to his.

On the other hand, Captain Conder appeals to the *chain*, and after

measuring up my plan, says (p. 82), "Mr. Birch's 'City of David' now occupies an area of 200 × 600 feet, or $2\frac{3}{4}$ acres. I do not consider this large enough for a city, or even for a country house and grounds."

I stated (p. 34) that I had made the area at least three acres, but it seems that my reduced plan is a twelfth of an inch too narrow, representing about 25 feet on the ground. It must therefore, to remove the discrepancy, be made a *line* wider, or *three* altered to $2\frac{3}{4}$ acres. The error no doubt arose from my being neither a draughtsman nor a surveyor.

As Captain Conder observes that my city of David *now* occupies this small area, I must remind him that this is the only estimate I have given. When I mentioned 10 or 15 acres (1884, 78) I was quoting *his* estimate of the area of Ophel (1883, 195), which he pronounced absurdly insufficient for the City of David; but I never intended to trench on his generosity to any such extent.

In thinking 200×600 acres too little for the castle of Zion, Captain Conder must have forgotten that in the "Memoirs" (Vol. I., 130) his plan represents the castle of Belfort (Kulat es Shukif) as still smaller. Sir Charles Warren's measurement of it was 400 × 230 feet at the most. That castle held out against Saladin for a whole year. Why should the Jebusites want more room than the Crusaders? It may, however, be that Captain Conder means that a castle less than three acres in extent, would never have been entitled "the City of David." There is little to guide one on this point, but in 2 Kings x, 25, "the city of the house of Baal" has been taken to describe merely the place where Baal's priests lived in or near Samaria; and Patrick (2 Samuel v, 9) observes, "The fort (*i.e.*, of Zion) was not wide enough to contain the whole court and all his guards and others that had occasion to come hither, and therefore David built round about it." I assert as a fact that Zion was solely on Ophel (so called), but its area is at present purely a matter of conjecture. I think 3 acres, or even $2\frac{3}{4}$ acres, sufficient; still there would be some advantages in its having reached nearer to the pool of Siloam.

In the *chain* Captain Conder has provided me with an effective weapon for destroying his own Acra theory.

A. He identifies ("Handbook," p. 333) "the Acra of Josephus with the knoll of the present Sepulchre Church," and (p. 354) says, "In the Lower City also, not far above Siloam, was the palace of the royal family of Adiabene (5 "Wars," 6, 1; 6 "Wars," 6, 3)." It will be seen on reference that Josephus says this palace was *in the midst of Akra*. As therefore Captain Conder admits that Akra was a hill and places "not far above Siloam" a palace situated in the midst of Akra, it necessarily follows that Ophel (so called) must be Akra, for the Upper City certainly was not Akra (being distinguished from it by Josephus. "Wars," V, iv, 1), and the only other hill within Jerusalem "not far above Siloam" was Ophel (so called), south of the Temple. Therefore Captain Conder himself becomes a witness to Akra being south of the Temple. If he still claims that his site for Akra, west of the Temple, is correct, I reply that it can hardly be described as "not far above Siloam," being according to his plan (p. 235)

not the twelfth of an inch, but *three inches* distant from Siloam; representing on his scale not merely 25 feet, but 2,500 feet.

I have already pointed out (p. 29) that the levelling of the Acra mentioned by Josephus is unsupported by and contrary to 1 Maccabees.

When Prof. Olshausen placed Acra south of the Temple, Dr. Robinson rejected this site because

(*a*) One of the western gates of the Temple ("Ant.," XV, xi, 5) led to the *other* city, which he took to be Acra.

(*b*) Acra was over against the Temple ("Wars," V, iv, 1).

(*c*) Acra was naturally higher than the Temple hill, and Ophel could not have been so (*id.*).

(*d*) Acra used to be separated from the Temple by a broad valley which the Asmonœans filled up, but no valley apparently ever separated Ophel from the Temple.

And (*e*) Thrupp afterwards added that the space on Ophel was too small to have contained the Lower City or Acra.

The Doctor ("Biblical Researches") deemed his objections unanswerable, and gathered that the Professor had resigned the game. Yet the Professor might have checkmated the Doctor in four moves, by replying thus:

(*a*) The *other* city was the Upper City.

(*b*) Ophel so called was over against the Temple, just as the tower ("Wars," IV, ix, 12) near the south-west corner of the Temple was over against the Lower City.

(*c*, *d*) The tale of Josephus about levelling the Acra is simply nonsense, and contrary to 1 Maccabees (p. 29), and so too with the valley.

As to (*e*) Josephus says nothing whatever about the area of Acra, while Thrupp and Lewin admitted that the part south of the Temple was "at the time of the siege the most important part of the Lower City." They allowed that Acra took in this southern part, but did not perceive that Acra was confined to it.

Some, however, are so fascinated with Josephus' story about Akra being levelled that they seem blind to its absurdity. Let me then by means of the *chain* test show the *impossibility* of Captain Conder's site for Acra.

B (1.) According to Josephus, Akra (the fortress) was razed, and the hill on which it stood reduced to a lower elevation than the Temple. Yet notwithstanding this, Captain Conder gives the level of the rock near the Sepulchre Church at about 2,480 feet, and puts the Sakhrah (the highest part of the Temple hill) at 2,440 feet, or 40 feet *lower* (!) instead of higher. No doubt his figures are correct, but his theory is obviously wrong.

(2.) Captain Conder considered 10 or 15 acres insufficient for my city of David, which was the Akra of the Maccabees and of Josephus. Therefore I grant to him *one* acre more, hoping he will consider 16 acres sufficient for his own Acra.

(3.) He says (1880, 81), "The amount of the hill lowered is about 30 feet on the average . . . and if, as seems not improbable . . . yet greater."

Multiplying 16 acres by 30 feet (I hope some R.E. or C.E. will check my figures), one learns, with no little amazement, that the Jews must have removed $16 \times 43{,}560 \times 30$ or 20,908,800 cubic feet of rock in carrying out Josephus' fancy.

A cubic inch of Jerusalem rock weighs (I believe) slightly more than one ounce, which gives quite 1 cwt. per cubic foot. Therefore, in levelling the Acra hill, more than 1,000,000 tons of rock (not to count the old material of the Acra Fort) must have been moved from its bed. Josephus states that the Jews were busy at the work day and night, for three years, but allowing them rest on some Sabbaths and feast days, we will reckon that they worked only for 1,000 days, so that the daily output would be 1,000 tons.

Cui bono? Was the game worth the candle? Captain Conder ("Handbook," 347) says, "The object was to make it impossible for any fortress built on Akra again to command a view of the interior of the Temple courts."

What! A Jew persuading his countryman to lift 1,000 tons daily for 1,000 days and earning gratitude thereby, and all only to prevent a view of the Temple being gained from the west, which any day might be gained from the north or east! Impossible. Gentile eloquence has cajoled many into foolish acts, but nothing like this. Why not without any fuss have raised the wall on the western side of the Temple? Or, if an eighth wonder of the world was indispensable, without more rock being quarried, a gigantic dry ditch might have been hewn out (as a northern defence to this troublesome Acra and Jerusalem), 2,000 feet in length and 100 feet in breadth and depth. And, as to fame, Simon would then have gone down to posterity, not as "a remover of mountains," but as "the Father of the Abyss." *Dulce est desipere in loco.* I have trifled enough. Let me now thank Captain Conder for the help of the *chain* in unmasking the outrageous absurdity of Josephus' most popular fiction.

In the anonymous paper on the Herodian Temple (p. 96), the writer seems to me to have forgotten that the excavations of Sir C. Warren amply *confirmed* Josephus' statements about the depth of the Tyropœan Valley.

That writer also falls into another error when he says "Josephus *incorrectly* assigned the position of the Acra." He indeed located it quite correctly, but his interpreters misunderstand him (p. 26). Again the "two gates" (Ant. XV., xi., 5) on the western side of the Temple did not lead into the Acra at all, but into the suburb, while two gates (and not one only) led to the Upper City, for one led to the royal palace in the Upper City and the other to the *other* city—*i.e.*, *the upper city*, as rightly observed by that writer on p. 96.

If any one still remains eager to emulate the prowess of Joab, let him attack Zion on Ophel (so called).

W. F. Birch.

THE NEWLY DISCOVERED ROCK-CUT TOMBS CLOSE TO THE JEREMIAH GROTTO NEAR JERUSALEM.

By Conrad Schick.

(*From the Zeitschrift of the German Society.*)

For some years the "Battlefield" with the neighbouring ground west and north of the Jeremiah Grotto has been the property of the Dominican Order; houses have been built there, the ground examined, and a high division wall erected on the road by the side of the hill of the Jeremiah Grotto.

When the foundations of this latter were dug, some tombs were discovered in the rock which had to be more closely examined and cleared, as the line of the proposed wall was to go straight across them. Their size was so great that it seemed desirable to preserve them in their entirety, and as they subsequently turned out from their hewn crosses to be Christian tombs, the Order regarded their preservation as a duty, and proposed to build a Sanctuary for them later on. The tombs were cleared out, and the bones carefully collected and interred in a suitable place. The place itself was covered by a protecting building.

The exact position of these tombs outside of Jerusalem is as follows:—

You go northwards for 245 metres through the Damascus Gate on to the road leading to Nabulous, and then turn east to the right into a small bye-way. After 40 metres you come to an old cistern, and perceive on the left side (north) a low gate which leads through the northern boundary wall of the road into a garden. Continuing for 50 metres in a northerly direction you reach the entrance of these vaults. It lies in the second bend of the above-named bye-road, where it again diverges from a north-easterly into an easterly course.

The tombs are entirely hewn out of the rock, and only in a few places, obviously at a later period, has masonry been added. Undoubtedly the tombs originally were of much greater extent, but have been destroyed by excavations carried on in the west. On closer inspection one easily recognises that their origin dates from the Jewish era, and they do not in any way differ from the Jewish rock-tombs so frequently found in Palestine, and especially in the country surrounding Jerusalem.

After their partial destruction and possibly complete desecration they were used by Christians as places of interment, and were considerably enlarged. I arrive at this from the fact that these enlargements have been carried out in a different style, and exhibit not only crosses cut into the rock, but also in high relief hewn out of the rock. The former might have been cut into the already standing wall surface; the latter could only have been added if the rock was newly manipulated or new vaults built. Also

in several places the doors have been subsequently made larger and notably higher.

The great vault with its side chambers is genuinely Jewish. It is 5·30 m. long, 4·30 m. broad, and 3·60 m. high. The walls are smooth, the ceilings horizontal, and the doors much higher than usual, being, namely, 1·80 m. The antechamber, going west, is 2·50 m. long, 2·30 m. broad, and, like the other side chambers, 2·30 m. high. It had suffered from the above-mentioned destruction, but was later restored with masonry and filled with tombs sunk into the ground. The side chambers, which lean to the west, have been preserved in their entirety, each having three stone benches for the reception of the corpses. The place for the dead was marked by a cushion-like elevation, formed out of the rock, into which a half-round hollow was hewn. The longer benches had this on both sides. Whether they were meant to serve for two corpses, or whether one could have the choice upon which side to lay the head, cannot be determined. The benches were 1 m. high, with the upper surface slightly hollowed and furnished in front with a low edge to prevent anything rolling off. Under the dividing wall and the contiguous stone benches of the side chambers there is an apartment hewn in the rock, which serves as a common grave. Towards the east there are likewise two side chambers, of which the southern, however, is only a passage to a third, and which has one sepulchre only.

In the eastern side chamber three sarcophagi have later been hewn out of the ground, and a new chamber constructed towards the east, obviously for the definite object of serving as a common grave. Of the western side chamber only the partition wall, together with the door, has preserved its original form ; towards the west and south the place has been considerably enlarged, and a quantity of tombs have been hewn on an entirely different system, besides which the missing rock is replaced by masonry. At the same time the proper entrance to the vault on the south side has been restored. In three places close together, from west to east, we find in the ground in twos (in the western row in triplet), sarcophagi of an extraordinary length, the lower being 0·50 m. high, and only 0·50 m. wide, whilst the upper is 0·51 m. wide. On both walls there was therefore a broad ledge of 0·15 m. broad, upon which stone slabs could be placed so that when the lower grave was filled with corpses it could be closed up. The tombs in the western row have the greatest linear measure, viz., 2·70 m., with a depth of from 1 m. to 1·20 m. Some of the sarcophagi are rounded in a sort of apse at one end.

On the north side of the western row, as well as on the southern side of all three rows, vault-like excavations are to be found 2·50 m. long, 1·10 m. broad, and 1·90 m. high, which served as common graves ; crosses are carved on most of them. On the above-mentioned ledge, in the middle division of the western row, two fragments of a sarcophagus still rest. Is the explanation of the extraordinary length of the tombs perhaps that they were primarily intended for the reception of stone or wooden coffins ?

Every sarcophagus was immediately covered with stone slabs as soon

as the corpses were laid inside, and most of them are to be found existing to this day. The rock walls on the long sides (north and south) of these three rows overlap the sunken graves 1·65 m. The whole enclosure of this peculiar group of graves appears to have been restored by means of small planks or laths. On the long sides hewn in the rock there are small furrows or rabbets running horizontally, by means of which one plank after another was let into the hewn groove in the side, and then finished up sidewards into the rabbet opposite, till a closely built plank covering was formed.

As the curve was only 0·20 m. broad, wooden and not stone slabs must have been used, as stone slabs of this insignificant breadth opposed to a length of 2·80 m. is inconceivable, and no remains of such are discoverable.

Whenever a burying took place, the requisite number of small beams were removed, and at the conclusion again inserted. A little staircase in the vicinity of the entrance led down from the sarcophagi to these vaults.

It is surely not by accident that the plan of these tombs, which are proved by the hewn crosses to be Christian, provides that the dead are much more closely united than is the case in Jewish tombs. Probably they wished to keep in view the idea of the close brotherhood of the Christian community.

In the rock covering of the principal chamber there is an opening which was no doubt originally intended to let in light and air, but appears at a later period to have been used for conveying the corpses through ; at any rate, exactly underneath, a large number of bones are to be found. In a deepening of the ground was discovered, we were told, a small meta box, which could only be opened by force, and which contained different bones of animals, and particularly birds. I did not myself see it.

In the sunken graves, which were first restored in the Christian era and closed with stone slabs, a plate was found bearing in Greek letters the word "Ephthemios."

In the vicinity of this grave chamber several hewn stones and broken shafts of columns were found, one of which was 4·30 m. long and 0·90 m. in diameter, and another shorter one 1·10 m. in diameter. Possibly further exploration of this and other remarkable wall remains at this place will yield still correcter conclusions.